Science in the Steady State: The Changing Research University

Contractor's Report

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OTA is concerned with the health of academic science in the U.S. under conditions of reduced Federal funding directed toward achieving a "steady state." Of special importance are the effects of this steady state on the research climate, research careers, and graduate and postdoctoral education. This consultant's report provides theoretical speculation, qualitative data, and interpretation to assist OTA in thinking about science in the steady state.

The report is divided into three main parts. The first part considers the underlying notion of interpreting science in systems terms and sketches the analytic categories necessary for that reconceptualization. Part two draws on face to face interviews with scientists to examine the possible consequences of the shift to steady state science. Part three offers a set of issues for policy and research and critically examines the idea of a steady state for science.

Much of the empirical material presented here was part of a paper presented at the AAAS meetings in February 1987 that will be published in Science and Technology Studies later this year. The work was explicitly exploratory, intended to uncover new relationships and suggest new interpretations and areas for policy attention. Thus, these results are suggestive but not definitive.

I. SCIENCE AS A SYSTEM

The steady state is an idea drawn from systems theory that assumes a model of science as a social system. Moreover, such a model would be helpful in understanding how and with what consequences a steady state may be attained and sustained. Although the Mertonian sociology of science rests on some systems notions, as does the functionalist theory which undergirds Merton's work, there is no fully developed interpretation of science in systems terms. Storer's (1966) volume on the social system of science is richly suggestive but insufficiently rigorous and far too limited in the types of actors and processes it contains. Perhaps more tellingly, writers in the higher education literature have noted with dismay the general disregard that sociologists of science have shown for the university as an organization and workplace (e.g., Schwartzman, 1984:200). And only recently has research funding, a salient component of the science system, gotten much attention in science studies (for exceptions see Cozzens, 1986; Rip and Nederhof, 1986). In effect, sociologists of science generally disregard the educational functions and organizational and policy contexts of science.

For these reasons we are very far from a model of the science system that might help in evaluating policy. Moreover, our base of knowledge about science is so sparse that such a model cannot readily be assembled from the literature. My contribution will be to call attention to some processes that deserve attention in a systems model but I will not propose such a model.

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The first step in developing a systems perspective on science is to define the system and demarcate it from its environment. This raises familiar questions, such as "What is science?" and "Who is a scientist?" But it also invokes more difficult organizational and institutional issues, such as incorporating sources of (private and governmental) research support in the model or describing the fit among science, education, and medicine or even establishing the place of science in society. This last matter is deceptively difficult, for science is sometimes viewed as a cultural end in itself and other times as a means to other valued outcomes (such as national security and economic prosperity). Hence, persons who disagree on the societal purposes of science often are talking about quite different sciences.

Science is a system that converts one sort of resource into another. The inputs include people, knowledge, equipment, money, and a form of moral energy reflected in the prestige of scientists within society. (The occupational prestige of scientists in the judgment of the general public typically falls just below Supreme Court justices and above members of the President's cabinet.) The chief output of science is certified knowledge; that is, knowledge accepted as correct at a point in time by the critical community of scientists. Science also contributes, in ways that are hard to specify, to our pool of scientifically- and technically-trained workers and to the store of know-how (technology).

Science comprises several subsystems, divided along two main dimensions. One dimension is intellectual, partitioning science into disciplines, specialties, research areas, and the like. Each of these has its own production process, and they are interdependent to varying degrees. The second dimension in organizational, composed of laboratories, universities, agencies, companies, and the like. The elements of this subsystem are also interdependent to a degree, and the two types of subsystems interact in many ways. In fact, one can only talk abstractly of disciplines outside the university context; in practice, neither the university nor the discipline would exist in the contemporary world without the other. (This is similar to the circularity in Kuhn [1970]: a paradigm may be a set of ideas shared by a scientific community and a scientific community is a collectivity that shares a set of ideas.)

Beyond defining the components of the science system, their interrelationships, and the flow of resources among them, it is also necessary to identify the systems functions of science. For example, feedback and feed forward have become everyday terms, yet it is very difficult to specify how they operate within science. For example, when the key problems in a research area are solved, how is that message communicated to the educational subsystem to reduce the flow of new scientists? Our theories of specialty development tell us that this occurs, but the mechanism is hard to specify. Or how do funding agencies communicate the need for new work and new scientists in an area? We need to specify these processes if science as a system is to make sense.

Morphogenesis and morphostasis, the systems forces for structural change and structural stability, are even more complex and difficult to specify for science. Because we don't have a good grasp of the forces at work on and within science, our view of change tends to be simple and evolutionary (e.g., see review by Bohme, 1977).

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Finally, the ideas of equilibrium and the steady state pose the greatest challenge, for they presuppose that the relationship of a system to its environment and its dynamics of change are well understood, permitting analysis of the mechanisms employed to sustain the relationship. For example, a system may change its structure to remain in the steady state:

> "Structure is never self-maintaining; a constant expenditure of energy of some kind is required to maintain any open system's 'steady state.' Discrepancies of one kind or another lead to continual remapping and reorganization....In an open system, then, the 'normal operation' of its institutions constantly generates an input of variety and strains thereby contributing to a continuous process of 'structure-elaboration' and reorganization" (Buckley, 1967: 130).

In this report I will outline some of the strains and structural elaborations underway in academic science in one area of research. While this is a purpose less grand than proposing an entire systems model of science, I judge our knowledge base too thin to support such theorizing. Instead, I will examine how scientists in one area of research have responded to changes in the funding environment. Developmental neuroscience is an especially good case for understanding what might happen en route to the steady state as it is an area in which the demand for research support and academic positions have far outstripped the supply. The effort to achieve a steady state through funding policy will probably entail similar reductions in research support and other resources for areas with similarly high levels of scientific interest and activity. Thus, while the results of this case study are not generalizable to other areas of science, the processes that emerge may be akin to those we will encounter on the way to a steady state. The essay will conclude with some analytic remarks on the idea of a steady state for science and some empirically-driven policy and research issues.

II. FUNDING AGENCIES, UNIVERSITIES, AND SCIENTISTS

A. The argument

Research money figures into academic life more prominently than before. Not only do scientists have their own needs for research money, intensified by the demographics of their research area, changes in federal funding policies, and the technical demands of their work, but these needs are amplified by the financial expectations of their universities. It is very hard to document, but many universities and departments depend upon externally-generated direct and indirect costs for a portion of their operating budgets.

These needs are communicated to scientists in several ways, especially in promotion and tenure criteria and financial incentives (in the form of raises, increments to salary, and discretionary research money). Pressures to obtain external research support are transmitted through departments and their faculty to postdocs and graduate students. Often they must obtain

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their own external support (as internal support grows scarcer) or be supported by a faculty member's research grant. The importance of external research support for universities is expressed most powerfully in the conditions of employment for a sizeable (and in some areas, growing) class of scientists whom I call "academic marginals." These positions fall into a nebulous area between postdocs and regular faculty, offering incumbents some of the intrinsic satisfactions of research in an academic environment but few of the extrinsic rewards that accompany faculty standing. Thus, obtaining research money is often an (unstated) condition for tenure (and continued employment for those not on the tenure track).

In effect, the ability to obtain research support has become a new criterion of success in science, similar to publication and citation counts in the sense that money is quantifiable and free from the subjective judgment of "quality" that arises from reading publications. Unlike publications and citations, money is a leading indicator because it is awarded for work to be performed, not work accomplished (although most scientists' claim that a successful grant proposal depends upon having done a substantial fraction of the work proposed.) Moreover, the heightened importance of research support and the sharper competition for it may be altering the conditions of employment for academic scientists.

Most directly, the need to obtain support causes discontinuities in research plans and can shift scientists' research interests. The chief accommodation appears to be from basic problems to more applied ones, but there is also evidence of movement toward "hot" new techniques at lower levels of analysis. And some scientists take funding probabilities into account as they fashion the broad contours of their research problems. These redirections might be desirable, but their force is not felt uniformly across all levels of academic employment but most strongly affect those in untenured or marginal positions. Thus there is some risk of a generational impact that will endure as these cohorts mature.

To make matters worse, these scientists who depend on the funding system often have difficulty understanding the rules that govern panels and study sections. Younger scientists complain that their elders play by different rules than they do and that the rules have charged over the years. And nothing in their training and experience has prepared them for the challenge. Experienced investigators, including study section members, mention that study sections hold them increasingly accountable to do the proposed research (at the expense of creative latitude) and that the system is utterly intolerant of error or inexperience.

In consequence, scientists have not only altered their research interests but have also changed their research styles. Principal investigators report that they now give their research teams less latitude than they had in the past, and less latitude than they had experienced in their own training and would find desirable today. This constrains graduate students' education and impairs postdocs' (and marginals') ability to achieve an independent career.

Increased accountability, tighter research budgets, and diminished discretionary resources from universities combine to affect research practice, diminishing the "bench quality" of research--the actual conduct of experiments and preparation of proposals--and encouraging fragmented

publication of scientific results. Scientists also feel less able to take risks in their work, because failure may cause an interruption in funding and an interruption in the career.

Changes in the science itself, such as the advent of molecular biology in the neurosciences, are hard to accommodate without some slack resources. Performance criteria on grants are so high, and funding agencies' standards for proposal quality and proposer experience are so strict, that seasoned scientists are having difficulty accommodating change. Some are caught in a double bind: unable to secure funding for their ongoing research because new approaches are more appropriate or more exciting, yet unable to receive funding for work with the new techniques because they have no track record and funding agencies can't afford to support people while they learn.

The overall impression is that as the academic science system, here considering chiefly universities, funding agencies, and academic scientists, becomes finely tuned for efficiency it will become less robust. Moreover, performance will increasingly be defined in narrow, conservative, measurable ways, perhaps in an effort to embrace a putatively objective selection criterion in straitened times. Perturbations of any sort may not be handled well. Academic marginals, those inhabitants of the "never-never land" between faculty and postdocs, may be a consequence of these hard times. Universities are unable to provide them positions, yet research funds provide them neither adequate work lives nor reliable paths to more satisfying careers. Instead, these scientists stand ready to provide skilled, cheap labor and plug short-term gaps in the faculty ranks.

In sum I am concerned with three types of actors in the academic research system: funding agencies, universities, and scientists. I propose that, if research funding is tightened to drive the system into a steady state, the following will occur:

Funding agencies will:
- avoid risk;
- not tolerate errors;
- rely on objective (countable) criteria;
- demand increased scientific accountability.

Universities will:
- establish new rules;
- experience value conflicts and ambivalence;
- create new structures and positions.

These will affect scientists':
- problem selection;
- relations with research team members;
- mentoring practices;
- research quality
- willingness to assume risk;
- ability to accommodate scientific change.

I would take much space to recast these into the language of systems theory. Instead, let me connect the preceding discussion of science to the earlier description of systems ideas by noting that funding agencies' and scientists' lessened willingness to assume risk is an example of feedback

processes and that changes in universities' rules, structures, and values are instances of morphogenesis.

B. Climate for Research

Scientists feel pressures, both subtle and blatant, to obtain external research support. Some of this pressure originates in the scientist's desire to maintain a research program, but some also derives from the university's need for research funds and the indirect costs associated with them. These pressures interact with a scientist's employment circumstances, particularly whether the scientist has a tenure-track job or has been awarded tenure, and with the actions and apparent inclinations of funding agencies, to influence scientists' research behavior. Those in marginal jobs (about which more will be said below) are most strongly influenced by these pressures, because interrupted support means an interrupted--or ended--career and an interrupted pay check. But regular, tenure track faculty are also strongly influenced by the need to obtain external research grants. This quest for funding colors their choice of research problems, the quality of their research, their decisions about publications and their relations with members of their research teams. The subsections below develop these influences and provide supporting detail from the interviews.

1. The Funding System

Funding agencies exacerbate the pressures on scientists to obtain research funding by sometimes making decisions that appear to follow "new rules" (or, perhaps, no rules), by demanding increasingly strict adherence to the proposed study plan and increasingly higher standards of productivity, and by showing very little tolerance for error or risk (more on risk in a separate section, below). Yet such changes in funding agencies' behavior may be expected during periods of changing priorities or in times of intense competition, when absolute distinctions (e.g., funded or not) must be made among similar objects (a lot of generally sound proposals). That is, as funding priorities change it will become harder for scientists to predict an agency's decisions and interests based on its history. Young scientists, unless they have been well-socialized into the new climate, will experience the intensified competition for funds as a disorienting change in the rules of the game. And experienced scientists will fret as the rules do change, increasing their accountability on scientific matters and decreasing their margin for error or vagueness in the proposal.

Changes in funding priorities and intensified competition for funds influence scientists' perceptions of the funding system. To some the research funding system seems irrational. One scientist had under review a training grant proposal and a center proposal. Although both had very fine priority scores, neither may be funded because of policy decisions to reduce training expenditures and a feeling that there are "enough" such centers already established. The effect of these decisions is to call into question, in this scientist's mind, the funding system's rationality and the fairness of its evaluation criteria. Others are concerned about "[the] whole lottery aspect [of obtaining research support]. Doing good work and working very hard are necessary but not sufficient conditions for getting your grant renewed." Both scientists quoted above are members of NIH study

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sections, with an insider's view of the evaluation process, NOT scientists to whom the system is a mystery.

Some young scientists feel that the rules of the game have changed and that they have not been taught the new rules. As one put it,

> "My biology professor grew up in the glory days of easy money....What I don't think anybody in my generation knows is that the rules are totally different now. Those 60s rules that the old guys play and the middle-aged guys play don't apply to us. There's no easy money and there's no toleration of eccentricity. In other words, you can be brilliant but a geek and still get money in the 60s--by that I mean you can have great ideas but write a bad grant--you'd still be given money. When I was a grad student the older generation would sit around and talk about which schools in northern California they would consider working at. That mentality gets transmitted down to the succeeding generations, but those rules don't apply. I've known people who've gotten screwed, including myself, because we've had those expectations subliminally, even though rationally we know they can't be true."

Others concurred, emphasizing that the least imperfection, the least doubt in the reviewers' minds, would be sufficient to drop a priority score out of the fundable range. This influences a scientist's willingness to take risks in a proposal, because a proposal that extends the proposer's competence may be flawed in some way or may be disbelieved because the scientist has no track record in the area. And a project that stretches his or her abilities may not work--may not yield the expected results--and thus may be in jeopardy of non-renewal.

Interestingly, when the "old guys" reflect on the "good old days" they do not recall a time of especially easy money. For example, a research physicist wrote:

> "I was educated and my research program developed during those supposedly halcyon days of the late fifties and early and mid-sixties. I do not remember research support being all that lavish then, as one reads today. Rather, that period was marked by an intense competition for federal and university research funds, but with a reasonably sure prospect that one could get sufficient funds to get the job done" (Orbach, in Phillips and Shen, 1982:44; emphasis added).

One major difference between his experience and the preceding scientist's lies in the emphasized portion: although he felt that adequate support to get the job done would be available, many scientists today are not sure they can secure adequate support to keep their own jobs. The second difference lies in the changing research university. As Wyngaarden (in Roberts, 1984) reminds us, in the fifties and sixties many universities were unwilling to accept faculty salary as a part of a research grant. This was thought to put the scientist's institutional loyalty at risk. Today the imperative is reversed, with many universities demanding that their faculty provide a portion of their salary through research grants.

For their part, study sections and review panels have an impossible task, for the discriminations they are called upon to make are often far more refined than the precision of their measuring instruments. One NIH official estimated that proposals rated within 40 points of one another were essentially identical in quality. Yet differences of far fewer points separate the funded projects from those that must be resubmitted. Perhaps in response to this need to make fine judgments on ostensibly meritocratic grounds--that is, after all, a part of the ethos of science--study sections hold scientists increasingly accountable for performing the work laid out in previous proposals. Yet this can be a problem for some. Here is one scientist expressing vividly his feeling of accountability to the study section and the difficulty that causes him:

> "As I tell my students you have to stand naked before the reviewers, you can't hide anything. If you hide anything they won't see it and they won't think you have it. You really stand exposed, give it the best, then you wait. By the time you wait, those ideas are gone and you have new ideas. If the money comes you say, 'Here, I have all this money but those ideas were for a year ago, a year and a half ago, and I have some more exciting ideas now.' So it's difficult to go back to doing the experiments that you very logically proposed to solve a particular problem when you have all these tremendously exciting ideas now that are somehow different from the ones you had proposed a year before. So I find it very difficult, and that's why, while I was funded for some of the grants, I started on other projects. I tried to do some of the experiments that I had proposed in the grants, but I tended to deviate. Some people are quite good and actually do all the experiments that they propose and somehow I admire them, they have that discipline, but I can't do it."

In fact, he had been criticized by a study section for not delivering on the implicit "promises" of his proposals. "On one grant the people actually went line by line...and said, 'Well, you didn't deliver. You said you were going to do this and you didn't do it.'"

Did that surprise you?

> "No....But I was a little taken aback because with that money I thought I had made a wonderful discovery and they didn't think so. They thought it was a way-out project...and I should have stuck to the letter of the proposal. I said I was going to synthesize this particular compound and I didn't do it. I said I was going to do so and so and I didn't do it. There is no appeal process...."

This happened to a renewal proposal, and this lack of accountability was the key reason the grant wasn't renewed.

Another scientist offered an insider's view of the greater level of accountability expected by study sections. When asked what it would take for her to insure that her grant would be renewed, she replied

> "Making significant progress on what I set out to do.

> That's something that's really changed. When I first got the grant it was to look at [two enzymes], but I found another enzyme and spent a lot of time on it. I asked the advice of a senior colleague and the department chairman when renewal time came and they said not to worry, a grant is not a contract."

But now she's on the study section and it has become apparent to her that applicants are really expected to do what they set out to do. She feels she would have to come up with something "very good" to justify not doing something what was proposed. Now this accountability is a problem for her, because she posed herself a technically difficult problem in her last proposal and has been unable to solve it.

In addition t appearing somewhat mysterious or anomic and increasingly insis ng that scientists produce what they've proposed the funding system also appears to have a low tolerance for error. This view was stated succinctly by one scientist, who said, "Now you have to be perfect, you have to write perfect grants--it has to be well-written as well as brilliant."

Others agreed that grant proposals must be "perfect" and "flawless," that a proposal raising the least doubt in a reviewer's mind is likely to fail. One researcher was very pessimistic about the prospects of getting a grant renewed. When asked why, she said, "I guess because I've been on a study section. I look at the grant [proposal] and I see mistakes in the grant, and I might make mistakes in the competing renewal and somebody might notice them." And any flaw would be fatal. This view is convincing on arithmetic grounds alone, for as priority scores and ratings crowd the top end of the scale, a very few mediocre scores will be sufficient to put a proposal out of the funded region.

2. Structural Change in the University

a. Financial incentives

Many universities rely on raising a substantial portion of their faculty's salaries through external research grants. I cannot support this assertion with financial data from the universities' accounts but instead can offer indirect evidence from published sources. For example, NIH research grants to U.S. colleges and universities increased from $677 million in 1972 to $1,009 million in 1983 (expressed in constant 1972 dollars). In the same period, indirect costs as a per cent of total costs increased from 22.3% to 29.1% (Institute of Medicine, 1985, pp. 192, 195). Thus a larger share of a larger sum is needed from research-oriented infrastructural costs. Without entering the debate about what this money buys, it is fair to state that the effect on research universities of substantially reducing the amount would be substantial. In other words, these are not fully variable costs that easily can be shed as a university's research effort winds down. In the same volume a labor dynamics model is presented and its parameters estimated to show what effect various levels of future federal funding and future enrollments would have on the growth of university biomedical faculties (e.g., summary table on p. 71). Leaving unstated the principles and assumptions of the model, the central message of the table is that biomedical faculty growth is far more sensitive to changes in federal funding than enrollments

(within the ranges they consider).

In my own research I have been most concerned with the incentives and evaluation criteria universities have established to reward faculty for obtaining such support. In effect, these policies give concrete meaning to the abstract processes implicit in the aggregate study. For instance, financial motivation interacts (and sometimes competes) with scientific criteria to influence a scientist's research plans. It also intensifies the already-keen competition among scientists for research support by urging more scientists to apply for money and by adding further rewards for success (and penalties for failure). Taken together, this organizational incentive to seek funding, in addition to the existing scientific incentive to do so, and the intensified competition it creates, may redirect scientific research and shape scientists' careers.

The university may express its incentives in a number of ways. Perhaps the most powerful way is to consider a scientist's funding record in promotion and tenure decisions, often without stating it as an explicit criterion but folding it in with assessments of the person's "stature" in the field. Every scientist I asked agreed that his or her department expected outside research funding, often to the level of providing a specific proportion of annual salary. Yet no, scientist could show a written policy that this was expected. A memorable case: when asked, one newly-appointed assistant professor immediately replied that he was expected to provide half his salary on research grants. When asked how he knew that, he paused, looked perplexed, and admitted that he didn't know why he knew, but then reasserted that he had to secure half his salary from research money.

Others are given the message more directly. For example, one assistant professor had just learned that his NIH project would not be renewed. He said, "At that time I already knew that the tenure decision was only one year away--and the chairman made it clear that without a grant it would be difficult to recommend me staying at this institution." In his department the lesson is clear: "The more grants you get the greater, speedier promotions one obtains, more space one obtains, more prestige." Conversely, "If you don't do that, you're out."

There are subtler incentives, such as allowing the scientist to use external funds to augment his or her salary, up to a specified limit. So, for example, faculty members in one state university system may increase their total annual pay (from the university) by as much as 75% at some campuses.

But such an apparently reasonable policy may be miscarried when it becomes entangled in bureaucratic measures of performance. For example, while one professor was without external research support he won a small fellowship in a campus-wide competition. He was directed to use the money for salary supplementation--to provide a portion of his salary not covered by his institution--rather than to buy supplies or pay for an assistant, because people in his department believe that the "Dean's office judges [faculty members'] success in finding outside funding by looking at people's salaries and seeing if they are generating their [share] or not." In effect, the ability to secure research support has become another surrogate measure of scientific performance, one that is closely allied

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with the organization's objective of obtaining more resources from the environment than with the scientific purpose of supporting a research program.

The university has available more subtle incentives for motivating scientists, such as "laundering" grant money earmarked for faculty salary through the university's accounts, then returning a portion of it to the department and to the investigator to be spent at their discretion. Such funds are used to support graduate students, seed new projects, buy equipment, and support foreign travel.

Finally, some scientists have been able to fashion jobs for themselves out of research funds, eventually parlaying several years of such support into a regular faculty post. This also attests to value universities and departments place on scientists' ability to obtain research funds.

Graduate students may experience the same sorts of pressures to obtain external support as faculty. In fact, often these pressures flow directly through a faculty member. For example, in one case a faculty member whose research support temporarily ended was provided with a grad student paid for by a departmental teaching assistantship. When the scientist succeeded in getting a project supported he was told to assume the grad student's cost, even though he had no funds for that purpose in the grant. He used supply money to pay the student for one year and told her to get an external fellowship to support the balance of her graduate training. In his words:

> "I kind of gave her a deadline, saying that if she's not able to find her own funding she will have to accelerate her research tremendously in order to graduate very quickly because I can support her for only one year, not two.... Trouble is, graduate students, especially at her level, take a very long time to generate any data and I didn't feel it quite right to pay her from the grant when she would presumably not generate enough results to justify subsequent renewal....But it worked out fine from her point of view" [for she got two fellowships].

Another professor insists that his students seek independent support for their training:

> "As soon as they come into my lab I say, 'I want a grant [proposal to NSF or NIH for a fellowship] from you, pronto!' I tell them, 'You know, I have to be funded. If I'm not funded I'm out of my job. If you're not funded you're out of a job, so better get a grant....That's the name of the game if you want to do anything in science now."

Yet another made the same point in somewhat different terms. He urges his graduate students and postdocs to "compete for funding--fellowships--as soon as possible because if they plan to be academics this is what their careers will be like."

At a more abstract level there is a tension between academic and research values that was explicit in two departments. One department's policy forbids postdocs to work with scientists who do not have research

grants, and it considered a similar policy for graduate students. They reason that without external research funds a scientist is doing no work that could responsibly involve a postdoc or student. The department has extended this "realpolitik" principle to its educational program, offering a seminar in proposal writing as part of the graduate program. (The course includes a mock panel meeting with open criticism of students' proposals.) Getting research support is an integral part of academic science. In fact, the pressures to seek outside support are so great that some tenured faculty "continually submit research proposals without success because it is expected of them."

In contrast, a second department acknowledges the role of research funding in academic science but sees a different problem and has taken a contrary stance. Here the faculty is trying to establish a policy for allocating graduate student support by some principle that will offset the attraction of outside research support. Their rationale is that the academic subject matter and its representation in scientific research should be independent, and that it is essential to insulate the discipline from current research trends.

In a third department, "[w]e accept a student, we make a commitment to fund them for one year [regardless of with whom they choose to work, thus the money is redistributed], then ... they either have to write a grant and get funded or somebody decides to fund them and they're really tied into that faculty member who doles out the money."

Although the issue of supporting graduate students is widespread, departments have resolved the conflict between academic and research values in varying ways. My data do not allow me to speculate about the relative frequencies of various solutions. I do wish to warn that we should be careful not to side with either the "academic" or the "realpolitik" position too hastily. As cold as the latter policy sounds, it does prepare its students for the competitive funding climate. And to the extent that "grantsmanship" contains tacit knowledge that is best acquired at first hand, this department attaches its students to mentors with proven skills. On the other hand, as reasonable as the first policy seems, it does perpetuate specialties that may be out of date and makes the allocation of students vulnerable to the internal politics of the department.

In some fields of science postdoctoral fellows are increasingly supported on research grants, not training grants, and become in effect employees of their faculty sponsors (NRC, 1981:137-9). This is not true of the biosciences (through 1978), but my data suggest that even those postdocs supported by independent training grants within departments are viewed by faculty as a departmental research resource. Only individually-obtained, external postdoctoral fellowships are viewed as truly independent.

These university policies intensify the competition among scientists for research support by urging more scientists to apply for support and increasing the stakes. But the competition for funds is already keen, driven by forces within science. In this category I include: demographic pressures on research resources caused by increases in the number of scientists trained in an area and migrations of scientists to "hot" new areas; higher production standards, driven by increased competition for

funds, which require larger research staffs on any single grant; increased scientific "overhead," caused by changes in accountability requirements on grants, increased administrative responsibilities caused by larger staffs, and of course increased amounts of time devoted to proposal writing; more expensive techniques which require larger staffs and more expensive equipment and reagents (e.g., tissue culture work); interdependence, especially in the life sciences, where many problems require a variety of specialized abilities.

The policy of universities to provide such direct financial incentives is part of a more general trend of nonprofit organizations (and some governmental ones) to run themselves more like businesses. These policies introduce a form of the profit motive and readily-measurable indicators of success into a once-nebulous environment. Also, if raising money is essential to the organization's success (or even survival), these policies can be viewed as a rational strategy for linking individual performance more closely to a key organizational outcome. Certainly these employees are motivated by higher pay, but they may sacrifice some of their intrinsic motivation when such pay-for-performance plans are instituted for they desacralize portions of the organization's mission and may raise issues of internal fairness that run counter to the members' shared values. And where science is involved the matter is even more complicated, because the value system of science--its evaluative criteria and their internalization and application by researchers--may become colored by financial considerations. For example, do you choose to tenure the scientist whose work is "better" in some intrinsic sense, or do you retain the more profitable scientist whose research has more practical benefits or brings in more money?

There are theoretical grounds for expecting such incursions of the "profit motive" from the private sector and the demand for accountability from government to continue for some time. Arguing in the Weberian tradition of increasing rationalization and bureaucratization of the world, DiMaggio and Powell (1983) proposed a process they called institutional isomorphism to explain why organizations linked together in an organizational field came to share one another's structural features. Following their argument, the increasing dependence of the university on government and the private sector for resources will cause universities to take on properties of those organizations (if only to meet their reporting requirements--the notorious OMB Circular A-21) and will cause its structure to evolve into a similar form.

b. A new role: Academic marginals

In recent years the number of available faculty positions in universities has been far smaller than the number of new doctoral scientists (NRC, 1981; Phillips and Shen, 1982). For a time the surplus was absorbed into postdoctoral fellowships and fellows were extending their stays in such positions as they waited for more suitable jobs to become available (Coggeshall and Norvell, 1978; NRC, 1981). In the basic biomedical sciences, for example, the number of postdoctorals grew rapidly from 1974 through 1982, then leveled off (although the number of Ph.D.s awarded in those years grew very slightly; Institute of Medicine, 1985: 56).

A portion of this overflow has been directed into positions at the

margin of the university: variously termed "the unfaculty" (Kerr, 1963), "unequal peers" (Kruytbosch and Messinger, 1968), and "the nonfaculty" (Teich, 1982), these scientists populate an academic "never-never land" made possible by the availability of research support and made miserable by the difficulty of obtaining such support and by their ambiguous status in the institution. Bearing the such titles as "Assistant Research Anatomist," "Research Associate," or "Research Fellow," these scientists typically do not share the academic rights and privileges of their counterparts on the "regular" faculty and may earn lower salaries. In some schools there is a well-defined career track outside the usual academic ranks for these people, so it is possible in some institutions to attain the rank of full professor (or its equivalent), although usually without tenure or membership in the academic senate (Kruytbosch and Messinger, 1968; Teich, 1982).

The numbers of marginal scientists have been growing rapidly in recent years. According to NRC data, academic employment for doctoral scientists in nonfaculty positions (other than postdocs) grew at an annual rate of 7.8% between 1973 and 1979; in contrast, faculty employment grew 4.1% per year during that period (NRC, 1981: 69). Contract researchers in British universities, a comparable category, grew roughly 65% from 1976 to 1984 (Pearson, 1987). And this category of academic researcher has attracted increased attention in recent years. For example, in the U.S. the NRC has undertaken a series of national studies of postdoctoral employment, careers, and research (e.g., NRC 1981). In the United Kingdom, contract researchers have banded together (into the Association of Researchers in Medicine and Science) and published the proceedings of a symposium that considered several aspects of their working conditions and career prospects (Roberts, 1984).

I believe that the growth of marginal positions (and the organized research units that often employ them) are a change in university structure driven by environmental uncertainties, new obligations (e.g., to industrial research), and ambivalence (e.g., the tension between educational and research values; see also Teich, 1982). Marginal positions and research units afford universities a degree of flexibility in personnel and administrative matters that the traditional faculty positions and department structure do not permit. Simply put, marginal positions are more readily emptied and reallocated than are faculty lines, and they do not have the faculty obligations to teaching and "scholarship." Research units do not have the organizational embeddedness and disciplinary anchors of departments and are therefore easier to dissolve.

If U.S. science moves toward a steady state I expect much of universities' growth and discretionary resources to be directed toward such positions and units. There are two main reasons for this. First, universities' initial responses to reductions in research support will be to reduce costs, operate more efficiently, and secure as much flexibility as possible so that shrinkage, if necessary, will be both possible and less painful. Marginal scientists are less permanent than tenured faculty, earn lower wages, and have more malleable research interests. Second, such units allow universities to build bridges to alternate sources of funding, such as industry and state government, by targeting a center to their interests.

Despite the apparently sound reasons for increasing the ranks of marginal scientists, these positions have certain disadvantages that affect the career prospects of incumbents, the types of scientific research that they do, and ultimately the scientific workforce. These disadvantages may be better understood by considering the circumstances of some marginal scientists in developmental neuroscience.

Marginal scientists are certainly a significant scientific resource. The importance of their research can be documented from their publication and citation records. Moreover, the work done in these marginal positions also contributes to the productivity of other scientists and indirectly assists academic search committees by providing a longer "track record" with which to evaluate job candidates. But their time in these marginal positions also has some costs which may be less apparent.

For example, the conditions of employment in such positions are often undesirable. The salaries are usually lower than academic salaries and the positions may not provide full-time pay (although they may demand full-time work). There is also little job security: employment usually terminates when a project ends. Moreover, such positions have limited academic "rights," such as claims on laboratory and office space or access to seed money and equipment. In one instance the scientist was not allowed to submit an independent research proposal, although she was permitted to have a new investigator award and an independently-run subcontract from a project at another institution. Underlying this prohibition were the department chair's inability to assign space to the scientist and his desire to push the fledgling scientist out of the (doctoral and postdoctoral) nest. Whatever the rationale for such decisions, they create difficult circumstances in which to work as a scientist.

These positions also entail substantial vulnerability, both within the institution and outside. Within the university such marginal scientists are dependent on others to provide part-time teaching or research employment to complete their salaries, and thus are indebted to those who bestow such favors. Even the allocation of lab space may be contingent on having a working arrangement with a more senior scientist and thus may be terminated without notice or recourse. In relations with the larger scientific community, occupants of marginal positions are at a competitive disadvantage because they do not have an established laboratory. They may also be more likely to have their ideas misappropriated from abstracts, papers, and proposals simply because they are less credible complainants and because they haven't yet staked their intellectual claim in the literature.

Perhaps most important, such positions typically offer poor career prospects, even for those scientists who accept the lesser salary and prestige and the inferior working conditions. Seldom are there clearly-defined career paths, and some who choose to remain are driven out as "too senior" to occupy such posts. And when there is a career path it is often not an enviable one.

For example, one scientist holds the rank of full professor, without tenure, and a rather high salary at a prestigious institution. Through several years' of very productive work, she had been promoted through the ranks. But her case serves to underscore the marginality of that career

path: precisely because of her experience, rank, and high salary it will be very difficult for her to move to another institution or to patch her salary together from a variety of sources. Not occupying a tenured position, she faces the prospect of leaving science before reaching 50 years of age if she is unsuccessful in getting her grant renewed.

The scientists I spoke with in this status are quite capable by most measures: Ph.D.s from very good schools, several publications, their work well-cited, and supported by the NIH and NSF. It should be stressed that these people are contributing to science, they are performing, not merely showing potential. Thus the positions are marginal to academia and perhaps also to the social organization of science (e.g., reviewers might be concerned about their access to laboratory space and consider their grant proposals somewhat differently) but as researchers they are not marginal to the intellectual enterprise of science.

Yet they are acutely aware of their precarious status. One said, "I had a feeling when I was looking for a job that I could have fallen off the bottom....it's a big jump from a postdoc to getting a job. That's the crunch time. Are people staying forever as postdocs? That must be pretty weird."

He is grateful to have a tenure-track job now, although he must provide 75% of his salary from research grants. And this is not without its stresses: "Because I'm in this... position I have a longer row to hoe than most--I have to be more perfect." Tellingly, he closed with, "I think it's the same position a woman might find herself in." He's absolutely correct, of course.

Some also experienced extreme bitterness and feelings of powerlessness: the classic symptoms of alienated workers. As one said, "I'm disgusted. I feel totally that I don't have any power and it can only hurt me to complain." However unfair her employment circumstances seemed, complaining would only increase the chances of reprisal (and perhaps dismissal without recourse).

Others mentioned that the system was unfair: their ideas had been stolen from proposals and submitted manuscripts, so they were paranoid and unwilling to communicate openly. Some complained that their educations were inadequate, leaving them unprepared for the grant-writing required and for the cut-throat character of research as they perceived it. They also felt that the necessary mentorship was withheld from them, that faculty members didn't help them publish a paper or obtain a grant.

This vulnerability is real, not merely "felt." Two respondents had their ideas stolen from proposals or papers under review but had no recourse. One couldn't persuade a senior colleague to take action, the other chose to do nothing because she feared a complaint would annoy the study section and perhaps trigger a reprisal. In contrast, their more senior colleagues usually admitted only some "wariness" of competitors and one full professor said he found meetings a wonderful place to work out his speculative new ideas.

Or consider the researcher described as a "sort of a super-tech...one of those people [to whom] you can just say, 'You know, I have an idea' and

she'll go to the library, and work out a lot of the details and discuss them with me, and we'll rehash the experiments, and she does the experiments very competently. She has a very good way of doing science." She sounds like an ideal collaborator, yet her labors may not always get appropriate recognition because the person she works with sometimes has "had a problem with other faculty members, [who] people don't believe that technicians should be on papers. On one occasion I had to drop her name out of a paper....I had a very hard time. It was either her name was dropped out of the paper or the paper was not published."

Perhaps the most important liability of such positions is their effect on the young scientists' access to collaborators. In developmental neuroscience and other highly collaborative areas of the life sciences these marginal scientists are caught in a double bind. Despite serious impediments to collaboration they need to work with others in order to be productive. But if they do develop a productive collaboration with a colleague at their current institution and eventually succeed in obtaining a "regular" job, then their collaborations are undone when they depart.

The problems marginals face in arranging collaborations are well-described by one scientist who had been in such a position. For him, the difficulties are

> "partly institutional, partly self-perception: you're so busy scrambling on your own research that you don't have time to take a breath. One of the nice things about getting a job [as an assistant professor] was that I knew I really didn't have to do anything for two years. It was expected I would have a dead time, and really I exploited that dead time to work on [his next area of research, a collaborative endeavor with a colleague at the institution]... But I don't think you have that kind of prerogative, psychologically or positionally, to set up those things quite as intensely if you don't have a job. For one thing, you may not be there long enough. The guy you're collaborating with has an investment too--we're both going to be here five years and it's worth our while to sink some time into it. If you're a postdoc or an overgrown postdoc everybody knows you're trying to get your ass out of there-- you <u>should</u> be trying to get your ass out of there."

So in the highly collaborative neurosciences these marginal scientists are likely to have a hard time finding collaborators. But there is a catch-22 even if they succeed, for once such attachments become an integral part of a scientist's work the cost of moving to another institution increases substantially.

One scientist had been successful in finding collaborators and put down scientific roots so deep that the cost of moving would be very high. Her next research project, for which she was preparing to gather pilot data, depends upon a difficult saliva assay and the use of nuclear magnetic resonance. Both are available in her current institution, which has a top-flight NMR facility and the world's expert in the required assay. To leave would mean abandoning the line of research because virtually no other institution could provide these resources and, since the project uses human subjects, it would be exceedingly difficult to conduct from a distance.

Others reported substantial but less dramatic reliance on collaborators and resources at their current institutions, sometimes as mild as the "halo" effect of a senior lab director and the connections associated with the position. In general, it is perfectly natural to expect a maturing researcher in a highly collaborative science to establish such connections--indeed, scientific research as well as the scientist's success probably depend on it.

These marginal positions may be important to academic science for several reasons. Most plainly, these are scientifically productive positions, occupied by energetic young people who spend virtually all their time on research. These positions also offer training and skill development--an opportunity to acquire the tacit knowledge of the craft, about which more below. Less obviously, these positions comprise a proving ground that assists in the assessment of new faculty and allow a measure of slack in the human resource system of science. For all these reasons the positions are very valuable. Yet the employment insecurity they carry is embittering, the career path tenuous, and the uprooting that will follow most successful job searches has high costs to the individual and the research system.

Marginal positions can be regarded as an extension of the scientific apprenticeship system, which includes graduate education and postdoctoral fellowships. But the marginal jobs fall far short of an ideal scientific apprenticeship. For example, such positions may not provide the freedom and resources necessary to become properly apprenticed as a scientist. The constraints, measured in terms of both budget and autonomy, are often so great that the appropriate skills are not acquired. Similarly, some marginals have suggested that the socialization is not adequate--that the right rules are not taught--thus failing at another function of the apprenticeship. Moreover, this altered scientific apprenticeship may not lead to secure employment.

More serious questions arise from the traditional use of apprenticeships to restrict the flow and shape the characteristics of workers entering a craft. In science the hiring of people into marginal positions (and their dismissal from them) may be less formal than hiring into faculty slots, and may carry fewer formal protections of individuals' rights. And whatever the formal guidelines for the treatment of such people on the job, it is very likely that informal norms and practices have substantial power.

These marginal positions are a variant of the postdoctoral fellowship, itself a hybrid of the German privatdocent and the English fellowship, in which scientists acquire skills, prove themselves, and seek faculty openings. Such positions are a product of federal research support and, as changes in federal research support are used as an instrument of science policy, these scientists' careers are redirected, perhaps in unintended and undesirable ways. In effect, we may be restructuring the academic career in a self-defeating fashion, removing some autonomy and many of the rewards from the early career while building in a career change at the wrong time. Or we may be restructuring academia, adding a new layer of professional whose rights and status are now under negotiation. A sensible science policy requires a better understanding of such marginal positions and their role in U.S. science and education.

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C. Scientists Reactions

1. Effects on problem choice

Only those scientists with the security of tenure can retain control of their research plans; others must be more willing to accommodate the research priorities of various funding agencies. They make specific accommodations by shifting from basic research interests to more applied, disease-oriented ones, or by attempting to work at a lower level of analysis, perhaps by using the fashionable techniques of molecular biology. They also make more general accommodations by leaving areas in which they have been trained or choosing a type of problem that may be unappealing but has good prospects for support. Thus, when money is so capable of redirecting scientists' interests we cannot be confident that those at work on a problem are best suited to do the work (rather than the ones most in need of the money) nor can we be sure that the "opportunity" costs of redirecting the scientist's attention from his or her current problems are reasonable.

One scientist was told that his tenure case would be much stronger if he were to secure more research support. He had a three-year project that was not renewed about eighteen months before the tenure decision. His response:

> "I obviously kind of dropped almost all scientific research at that time, [because I needed to write proposals and because] I couldn't afford keeping the people I had.... I put all my energy into trying to get a grant.... I thought up different projects altogether...for purely pragmatic motives, basically going through the institutes and wondering who's got the most money."

This effort succeeded admirably, so now he has two major funded projects that do not overlap very much at all. But even he terms it "an excess of money" and notes that "I was literally not prepared to run two scientific projects in parallel...."

Not only has he gotten himself a surfeit of money that exercises his ability to manage research rather than to conduct it, but he has also gotten into "essentially a measuring type of science...let's take a type A animal and a type B animal and measure as many parameters as we can and see if anything is different....that type of research I don't particularly like and I proposed it simply for the reason of obtaining money to do research."

This theme of moving from basic research into less challenging areas of applied work was echoed by others. Another young scientist redirected her work from basic developmental neuroscience to aging with some success, but said "development is still where my heart is." Both plan to move back to their basic research interests when possible.

In contrast, a tenured associate professor who had been without research support for a number of years (after a well-funded early career) "looked around and said there's the NIA and maybe I should get into aging....[But] I believe if I do what I want the money will be there from

some source or I'll get it done anyway....I would rather do this project with no money than do something else with more money. Right now this is where my heart is."

Another newly-tenured associate professor who was without funds decided to prepare a proposal for a major foundation but aborted the proposal because, "I didn't want to get money just for the sake of getting money, that would have diluted my research effort too much."

Other accommodations to the funding environment are less abrupt, involving a conscious decision in advance of a funding crisis. Despite their apparent smoothness these transitions involve substantial dislocation. Here are two contrasting examples, one a scientist who has moved away from his postdoctoral training and research inclinations, a second who has labored to follow the research example set for him by his mentor.

The first scientist had worked on a tissue culture study during a postdoc with an eminent scientist. He "would like to continue research of that nature, but very shortly after arriving here I realized how expensive this kind of research would be and that it would also require a long-term funding commitment from some source or another...it was not feasible for me to do this kind of research for me at the junior level.... [H]ad I been in that field the lack of funding [he recently experienced] would just about have totally destroyed me. I would not have been able to continue that research at all."

The second calculated his risks and preferences somewhat differently, choosing to avoid highly competitive areas and instead working on esoteric research problems which afford him more latitude and excitement in his work at the cost of increased difficulty in obtaining funding and getting his results published. When asked where he acquired this preference, he replied:

> "Maybe I was influenced by my advisor. He was in a very highly competitive area and left it....I watched him pick a very esoteric field, he carved a little niche for himself, and he enjoyed it very much. I think his blood pressure was normal and he seemed very relaxed. I like to be very relaxed. I think competitiveness somehow cuts into your creative endeavors. You have to do something when you're very competitive, regardless of whether you think it's going to be productive. You feel obliged to do certain types of experiments or you will be beaten to the punch. You can't take very high risks. You have to go step by step, make sure you get something accomplished otherwise you'll show nothing because somebody else will have done all the work."

But he has had some difficulty convincing study sections to fund his "esoteric" research and has been criticized for changing direction in the course of a project, using grant funds to pursue leads which appeared important to him.

Although the two scientists made opposite decisions about the general direction of their research, the first eschewing a sound line of work because it seemed impractical, the second embracing his advisor's

philosophy of problem selection despite its practical difficulties, the common message is that it is very difficult for scientists to realize in their own careers the lessons of their training.

In general, then, some scientists will be more susceptible than others to the influence of changes in funding policy. These are not the weak-willed or incompetent, but those in organizational positions and career stages that make them more dependent on obtaining funds. Some of these scientists may experience the shift in support as a crisis, perhaps occurring at the inopportune time of the tenure decision, while for others the pressure may be experienced earlier in the career and may effect a gentler redirection of research.

2. The relationship between PIs and their research teams

Everyone acknowledges that obtaining preliminary data is essential to getting a project funded and launching an academic career. There is also general agreement that junior people must "win their spurs" by doing independent research. Despite this, there is evidence that tight budgets with little discretionary money, sharp competitive pressures (with high productivity standards for remaining supported), and high levels of accountability are impeding young investigators' movement from their work in others' labs to their own independent careers. Thus the interests of postdocs and students--who need some time to do their independent work under another's supervision--are not in harmony with the interests of the senior investigators who employ them (and who need a pair of skilled hands to keep the lab's output high).

The issue is complicated because postdocs and graduate students are both "research labor" and students, providing a skilled workforce while they learn. Yet as they're moved from training programs onto research budgets (as training programs end and university fellowship funds are strained), the emphasis shifts from their education to their research contributions. The potential effects of this are a reduced capacity for independent work (because they become important parts of a scientist's research effort early on), a diluted education because they acquire practical responsibilities early on, and premature specialization as they acquire the restricted range of knowledge and techniques required by a research project.

For example, three scientists relied heavily on the experience and talents of senior postdocs to carry out the day-to-day work of their labs. In fact, some carefully chose very experienced post-docs to be sure that the requisite managerial skills were there. (In one instance, "very experienced" meant twelve years of postdoctoral experience.) Yet one senior investigator fired a long-term postdoc at the urging of a funding agency that said the postdoc had been in that status too long. Thus the PIs are dependent on the advanced skills of postdocs in getting the lab's work done, without much provision for advancing the scientists' careers. Moreover, it is in the nature of these positions that they don't offer any advancement opportunity (in the academic sense, though one might be promoted from assistant through associate to "full" research anatomist in some places) and, increasingly, the PIs cannot offer their charges the scientific freedom and resources that might help them get a faculty position.

Some PIs were troubled by their need to be so restrictive. For example, one PI once allowed his staff much latitude to do their own work in his lab and to participate in decisions about his projects, but now he doesn't. Instead he feels obliged to squeeze out all the effort he can on his own work, often replying to their ideas, "That's very interesting, but don't try it on my time." He had been scared by his experience on an NIH study section, where he saw senior people with long-term projects lose their awards. He doesn't want that to happen to him.

And an assistant professor who also was trained by eminent scientists experienced a similar problem. When asked how much latitude he allowed his staff he replied,

> "Not very much. I used to do that during my first grant period. As a result I was accused by the reviewers of my scientific endeavors not being too focused, going in too many directions at once....so I try to keep everything within those experiments proposed in the grant or something extremely related to solving the problem under study." He was driven to this policy not only by his accountability to the funding agency but also by the productivity demands of a tight time frame: "I had to justify what I was doing within two years of initially getting the money; it was too short a period for this free-running approach to work."

But the more directive approach has costs too. First, "the likelihood of finding something interesting or important is somewhat lower." Second, the more directed approach "delays [the grad students'] educations in essence because I would say that the directed approach is them being more like technicians. Probably the value of their education is their ability to master techniques rather than necessarily to find out how to think, how to formulate their own problems...they will presumably do that at the postdoc level or as first-time faculty members."

One newly-tenured associate professor found himself caught between the academic values instilled in him during his training (with an eminent scientist) and the requirements of research. Whereas formerly he valued the quality of scientific work above all else (and was so slow to publish that it cost him his contract renewal at a prestigious institution), he says his motto now is "I don't want it done well, I want it done Tuesday."

The completeness of the academic role is being differentiated somewhat, with academic research and teaching giving way to a more complex division of labor, a greater delegation of responsibility. We can see this in the scientist with two concurrent grants who has become a manager of research, in the faculty who rely on postdocs to manage day-to-day issues in the labs, and in the delegation of teaching responsibilities to postdocs, marginals and grads. For example, while I was interviewing one marginal scientist she was interrupted by an undergraduate honors student whose thesis research was under the interviewee's direct supervision. This has consequences for the socialization of these new scientists and it complicates the lives of the marginals, postdocs, and grads who find themselves subject to many of the cross-pressures that faculty have lately acquired and have lately attempted to shed.

Or consider this assistant professor's management of undergraduate research in his lab:

> "Taking care of these undergraduates is a full-time occupation. Initially I used to do that, I used to do experiments with them, and I just spent too much time with undergraduates and I thought it was a waste of time at the end. For what I got out of it, for what they got out of it, I didn't think it was worth my efforts. And I did put a lot of effort with undergraduates initially. People who had no experience in the laboratory were, in the end, doing very sophisticated experiments. It's a full-time occupation, taking care of these students until... I can trust what they do. Basically that's the bottom line: if they do something and they show me a result, do I trust it? I have to be with them a long time in order to trust them. There's always some doubt. If there was some doubt I just said, 'Not worth it.' So I dropped it and I basically let the graduates take care of the undergraduates, unless there's a particularly unusual undergraduate. If I see some unusual talent I will spend the time with them. If I see that they're good material for graduate school, not for medical school...."

One might contend that this scientist is paid for undergraduate teaching and therefore is shirking his responsibility by pressing his graduate students into service as undergraduate teachers. But I think this is an unfair judgment. The point of my argument is that the faculty role has grown quite complicated and that a new set of criteria for success are supplanting (or supplementing) the old. Scientists are serving the interests of science, the funding agencies, their universities, and (as educators) of students as well. These demands contain internal inconsistencies and are at odds with some of the traditional values of academe (such as the intrinsic worth of educating students). These contradictions create ambivalence in scientists (who are undecided about which value to pursue) and role strain (by confronting scientists with inconsistent demands).

3. Research Practice: The Bench Quality of Science

Much of the bench quality of science--the nature of the results produced and the form in which they are reported--is influenced by the scarcity of discretionary resources and the need to appear productive in a measurable way. Preliminary data and replications, for example, serve to make more convincing arguments to others and to oneself. Also, more comprehensive publications offer the scientist more room to integrate results and speculate about their implications, as well as reducing some of the clutter in the literature. Yet tight budgets and competitive pressures remove some of the resources needed to do this, the former by reducing discretionary support, the latter by redirecting scientists' energies into producing a greater number of publications. These pressures were widely felt and clearly expressed by several scientists.

a. Research work

Problems arising from the absence of seed money were cited by two

scientists, one a marginal scientist whose position lies outside the usual academic hierarchy, the other a tenured associate professor. These cases are important for two reasons. First, they indicate that scientists may be unnecessarily in jeopardy of not being supported because they are unable to conduct preliminary studies that would strengthen their proposals. Second, the quality of their proposals suffers in a very concrete sense, for a proposal supported by preliminary research can be more cogently argued, more insightful, and better-planned (because there are fewer uncertainties) than a proposal written without preliminary work. Taking the funding agency's point of view, a project supported by preliminary work faces fewer unforeseen difficulties and has better prospects for working smoothly.

The marginal scientist's research plans led her to a study that would require the use of NMR to compare two different categories of patients. To strengthen the proposal, she hoped to run ten cases in each category, showing that the predicted differences are in fact observed. An NMR facility is available at the institution and she was able to negotiate a reduced rate for her pretest ($75 per patient, provided she would write the facility into her research grant to do those patients at $600 per case). Despite this, funds were not available to do the pretest and the proposal will include only a single NMR image, a token demonstrating that the equipment is available and that the proposer can interpret the image, but a far less persuasive argument that could be made by comparing a small sample of cases. The tenured scientist changed research interests in mid-career and wrote a proposal to work in the new area that made an analogical argument but lacked any data at all. He realized that preliminary results would have strengthened his case but, lacking seed money, was forced to advance the weaker argument. His proposal was funded, although its priority score was very near the borderline, and he has been very successful in his new area.

The availability of research support affects the quality of science in other concrete and mundane ways. Some experiments can't be done because they're too expensive, "So the result is that...you rely on a very narrow series of drugs--those that are cheap--that give you a good indication that you're probably right in whatever you're describing but certainly do not prove beyond a shadow of doubt that is the case." Another scientist economizes in his experiments, using 2 or 3 instead or 6 or 7. This affects the quality of the journal that will publish his results and makes him more dependent on the luck of the draw of referees to get his work published.

Others have noted that the character of research publications has changed:

> "Most publications have gotten shorter; the number of experiments that is contained within them is far less; the amount of information is almost minuscule; and there's very great tendencies of people not having done quite the right thing: not used large enough samples, not worried about some of the variabilities that are responsible for the results that they get. So in essence almost anything that is reported now has to be redone by others several times before it's accepted as the truth."

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This fragmentation may be caused by "...the immense pressures by university authorities and by the funding administrators for proof that one is making progress. And since progress is extremely difficult to measure in terms of quality, since that is a very subjective judgment....the other measurement is that of quantity."

b. Risk

"Quality" in science means more than the straightforward quality of bench work described above; it also includes more speculative flights of imagination, intellectual leaps which, at the time taken, were hardly assured of success. Changes in the funding environment may also threaten this type of quality by reducing scientists' willingness and ability to undertake risky projects. This might happen if the costs of failure become so great that few can afford to take a chance on an idea that may be sound but does not assure success. Moreover, this aversion to risk may become institutionalized, resulting in study sections and agency officials unwilling to support a project that has some chance of failure. (The presentation of preliminary results, discussed above as an element of the quality of science, serves to allay concerns about failure. In the limit, some say, it is ideal to complete the entire study, or at least its first year, before submitting the proposal.) Thus the quality of science depends in some measure on scientists' ability to take risks and absorb the consequences of failure. For their part, scientists carefully assess their risks, albeit without benefit of calculations, and can talk in detail about the riskiness of their work.

One aspect of risk is that of coming up empty-handed in a project. For example, one scientist regretted posing so technically difficult a problem in her recent NIH proposal, for she was having trouble getting good results. She was at the point of cutting her losses and resigned herself to publishing an approximate solution, based on data that were not very good in order to salvage some "yield" for her efforts. Because of this she felt her grant renewal was in serious jeopardy.

Too little risk also can be dangerous. One scientist had been very conservative in his past work:

> "I was doing experiments where I was very confident they would work out and fairly confident what the answer would be....It was always sort of good, solid work--sort of a good round steak--but then after a while people got bored with round steak and actually I did too. The work has held up but it just hasn't been the breakthrough stuff. It hasn't had the impact."

Unsurprisingly, most scientists try to balance their risks by choosing problems that allow them some "bread and butter" publications while holding out the possibility for the "breakthrough stuff" or by selecting a research strategy that gets at the fundamental scientific question while allowing substantial productivity. One assistant professor sketches this second strategy as he warns scientists about

> "Trying to go after a difficult problem which, if attacked directly, one might come up empty-handed. For example, had I remained in tissue-culture and tried to attack [my research

> problem] from that point of view ... I might have come up empty-handed. [But by] attacking the problem in a somewhat more indirect way I can generate data of interest to the scientific community. Nevertheless, even if I do succeed and characterize very well the appearance of the different receptors at different stages I still don't solve the basic mystery of the question I've set out...I will not discover the reasons why those synaptic connections are formed."

In other words, by choosing an approximate rather than a direct approach to his problem he is certain to be productive, to make a series of incremental contributions to knowledge, although this approach will not yield the definitive solution.

Study sections and panels may not be prepared to evaluate strikingly new ideas and results. One scientist had made a breakthrough and in fact had a report of that work in press at a very prestigious journal. Yet he decided not to mention those results in his renewal proposal but to rely instead on proposing to do "more of the same" for he feared that the study section would not believe his work and not support his proposal. His strategy was successful.

4. Accommodating scientific change

These tight financial constraints have also affected scientists' ability to adapt to changes in science. The advent of molecular biology in the neurosciences is a good example of this. Virtually everyone I spoke with has been affected in one way or another by the advent of molecular biology in the neurosciences, so scientists' preparation for the shift of investigation to the molecular level was a recurrent theme in the interviews. Very senior scientists with long research records (and funding histories) were just as concerned as their younger counterparts. Some of the established scientists expressed frustration ("I can't keep the terms straight.") and tried a variety of strategies to accommodate the trend. Some relied on getting collaborators or postdocs with the needed skills into their labs. Others tried to "bootleg" some molecular work on current projects, thereby obtaining preliminary data, establishing credibility as investigators in the area, and gaining familiarity with the methods and results. Their motives for doing this varied. Some felt that their research areas were "ready" for molecular approaches, and were driven to the molecular level by desire to contribute.

Others noted that study sections "wouldn't even look at proposals" that didn't contain molecular biology. Or felt more subtle pressure, as did one scientist who felt his proposal was not getting "as favorable a hearing [as it deserved] because it was not molecular enough....One of the major criticisms of it was that it was still [after repeated revisions] too descriptive and wasn't getting at causal mechanisms... nobody said, 'You'd better start doing molecular biology on your system' but I think that's what they were getting at." His perception may well be accurate, for two scientists who served on study sections and were comfortable with molecular biology said plainly that they pushed strongly for such work in their evaluations of proposals, and an agency official explained the steps he took to insure that molecular biology was represented among his panel and reviewers.

Some scientists even presented "five year plans" to move their work from its cellular version to the molecular level. For example, one young scientist has decided to shift his research to the molecular level because that is what twentieth-century biology will be remembered for and he wants to be a part of it. In his judgment,

Others point to past difficulties they've had in convincing federal agencies to support them while they begin work in a new area. Scientists are faced with a dilemma: their work must be sufficiently innovative to generate enthusiasm but tame enough to promise results. Risk may be a necessary element of good science yet the system that evaluates proposals is risk-averse and their employment circumstances are intolerant of failure. And they must somehow manage to keep up with technical changes in science while remaining funded, although funding agencies are reluctant to support people as they enter a new area.

This extended account from a scientist captures the frustration well.

> "I think the funding environment from the government doesn't give you much elbow room. If I had written a grant on that [topic], they would have probably come back and said, 'he doesn't have any experience, any track record, in this line of work, and so I don't think we can give him funds.' And probably since I hadn't been doing it as well I probably would have made some little error, maybe big error, in writing the grant proposals. And the system for reviewing grants at the federal level is extremely criticism- sensitive: you get any kind of criticism that potentially calls the research into question at some level it knocks your score down and hurts very badly." Even if the core idea is very good, and many times the criticism comes not because the idea is incorrect but because the reviewer hasn't understood what you've proposed. In fact, that happened to me on the last go-round....The study I proposed was designed to get around one particular problem that always creeps up in these kinds of quantitative studies, and we did it in a way to exactly avoid that problem but it didn't come across to the reviewer. And it was about the only substantial criticism...and it got dinged for that reason. Fortunately it survived...by the skin of my teeth, I feel."

III. CONCLUSIONS

A. Empirically-based policy issues

Changes in the university research climate raise several major issues for policy makers. The first set has to do with the plight of scientific marginals.

1. How many marginals are there? Do the numbers vary from one research area to another and why? What are these scientists' contributions to science and how do they differ from those of their colleagues in more traditional positions? Do they serve academia by providing a reserve army

of scientists to accommodate academic growth? If not, then are these positions merely a way station on the road to nonacademic employment or even nonscientific employment?

2. Is the number of marginal positions growing? Do we want it to grow and, if so, in what ways? Given stable or even declining enrollments, universities might be reluctant to increase the number of tenure-track faculty. At the same time research priorities may create a demand for doctoral-level scientists in some fields, and universities may respond by hiring scientists in these non-faculty ranks when there is a need, then firing them when the demand is met. Although this practice may benefit universities it may not be good for science or for the scientists involved.

3. Given the answers to the above, should universities take steps to limit employment in marginal positions? Or should the positions be institutionalized as an alternative career path? Should funding agencies support people in such positions?

4. Are we restructuring the academic research environment in a way that trains young scientists to be incapable of independent work or that provides too-strong incentives for acquiring specialized technical skills?

These issues may be addressed through the standard survey methods of the NRC studies (cited above), augmented by gathering data about the scientists' publication records and by interview or questionnaire survey studies of the universities' policies and procedures. It would be best to develop a short-term longitudinal study, covering about five years with three waves of face-to-face interviews. Such a study would uncover process through which young scientists are sorted into positions and the role of subtle peer and mentor influences.

We should also be concerned with changes in the structure and culture of the university.

1. I hesitate to raise this issue in light of all the current writing about the crisis in higher education, but we should re-examine the relationship between teaching and research. Do these two functions entail divergent values? Do they demand people with different credentials: a teaching license and a research license? Are students' educations compromised when they are placed in the dual role of student and employee, or do such opportunities to address "real world" problems (grounded in basic science or industrial need) enrich education?

2. A related theme concerns the growth of organized research units on university campuses, their contribution to the university's mission and to scientists' work. What forces are driving the creation of these centers? How do they differ from scientists' traditional work environments? What are the consequences for research of working in a center versus an independent departmental lab?

We have many sketchy studies of research centers but none that defines the populations, constructs a theoretically-grounded typology, and examines the roots of each type of center. In addition, more micro-level, longitudinal studies of centers' origins and effects on the process of scientists' work as well as its products are needed.

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The effects of funding policies on scientists' careers and research areas' viability are also needed. Which scientists are most likely to respond to an increase or decrease in research support? For which research areas are such changes most consequential? To some extent such questions can be addressed cross-sectionally, by comparing research areas and scientists' within them. But a more satisfactory study would follow a set of research areas over time, periodically interviewing scientists about their research plans and the activities of their labs. This design avoids the pitfalls of recollection and the incomparability of research areas. Moreover, should there be a concerted effort to change the research terrain through funding policy, a longitudinal evaluation study should be initiated to track the effects of this "natural" experiment. AIDS research may be an excellent site for such a study: where are AIDS researchers coming from? What are their qualifications? What sorts of problems have they set aside to take on this new line of inquiry?

We must understand the implications of funding changes for the quality of scientific research. If indeed both scientists and funding agencies become risk-averse under tightened funding, then the consequences for innovation could be grave. This is perhaps the most ambitious area of research in the list, for it requires measures of the character of scientific work and research plans.

Finally, we should ask how mature scientists will be able to stay current in the more constrained funding environment of the steady state. As universities and funding agencies try to run "leaner" in a constrained environment, and as the agencies increasingly hold scientists to high productions standards closely defined by the grant proposal, there will be decreasing flexibility for scientists to acquire new skills and gear up to enter new problem areas. Moreover, as departments increasingly rely on research support, not training funds, to support graduate students and postdocs, it will become increasingly undesirable for senior faculty to take time out on sabbatical leave or to work without funding for a period while they prepare to enter a new area. In the limit, we may reach the paradoxical point where established investigators are able to secure support only to plow familiar furrows and are unable to take time and resources to move into new ones, while new investigators, with fresh ideas but without a track record, are decreasingly able to obtain research support.

B. Conceptual issues in attaining a steady state in science

1. Defining the steady state

The steady state must be defined with respect to some particular relationship with the environment: Is it knowledge production, manpower development, resource inputs, or economic outputs that is to be "steadied"? The answer matters greatly. For example, a constant level of knowledge output may require increasing levels of resources. Whereas constant resources will lead to declining output due to secular increases in the cost of animals, reagents and instrumentation. Intellectual forces that are increasing the complexity of scientific research and the frequency of collaboration also contribute to rising costs and declining output per dollar of input. (The preceding discussion treats the outputs of science

as if they could be measured as accurately as the "widgets" of economics. They can't. But that matters little for the abstract discussion; it simply makes it impossible to test.)

While most analysts treat the inputs to science as the resource to be steadied (e.g., Phillips and Shen, 1982) there is no necessary reason for this decision. In fact, from a policy perspective it may be more sensible to ask what level of input is needed to sustain a desired level of output (measured in terms of scientists or knowledge production). Defining the steady state in terms of inputs rather than outputs ignores changes over time in the transformation process--the "efficiency" with which science converts its inputs into outputs--and threatens to vitiate the entire function of the system. It's a bit like planting a vegetable garden and establishing in advance the constant amounts of water, light, and fertilizer the gardener will provide, without regard for the amount and character of produce desired.

All this is made more complicated when the single idea of a "science system" is replaced by the reality that universities, disciplines, scientists are members of more than one system: at minimum, they all belong to the education system as well as the science system. While there is substantial overlap between the goals, functions, and resources of these two systems there are also significant differences. (Differences that are experienced by any professor who tries to balance teaching and research responsibilities or any administrator trying to allocate money and space between instructional and research use.)

Just as <u>individuals</u> experience difficulties when their social roles place heavy or inconsistent demands on them, so too are <u>organizations</u> and <u>systems</u> strained by multiple and inconsistent claims on their energies and resources. We might be provoked to ask again, with Philip Handler, "whether research and education should be as tightly bound as we usually state it to be in the model of the teacher-scholar" (Omenn and Prager, 1982:24). But the question should be posed at an organizational and systemic level as well as at the level of the individual scientist.

We find universities today serving the research needs of national defense and economic competitiveness, the knowledge demands of disciplinary development and articulation, and the educational requirements of society. If resources are reduced in an effort to achieve a steady state, the system may respond with a series of accommodations, one of which is an effort to use resources more efficiently. One likely consequence of the quest for greater efficiency is a brittleness that deprives the system of its ability to serve two goals at once. In short, it is unlikely that universities can be fine-tuned to serve both sorts of needs with equal efficiency. (Funding agencies are also demanding greater efficiency and accountability on their own, which exacerbates matters.)

Stated somewhat differently, to define a steady state in terms of inputs to a system overlooks the products of the system, its reason for existing in the first place. But simply defining the steady state in output terms isn't enough when the system produces more than one type of product (say, credentialed students and certified knowledge) and when each product varies in both quantity and quality. Under such circumstances it is hard to establish the desired balance of products and their properties.

Worse, an overemphasis of one or another product (say, research over teaching) may have caused the system to accommodate itself mainly to the one function. Ironically, the ability to serve both education and research simultaneously, the exchange of some efficiency for higher quality, and the support of excess capacity in the interest of adaptability (say, to national priorities or changes in the character of science) may be highly desired features that are jeopardized by a quest for a steady state.

2. Is a steady state possible?

Is a steady state possible for science or is science, in its nature, in need of growth? That is, perhaps science is similar to a developing organism (or the garden mentioned above) that may grow or wither but cannot be sustained with constant levels of nutrients. If this is so, then we should ask if it is necessarily so or can the system itself be changed to one that can survive with constant inputs? After all, the organic analogy may describe science but certainly does not determine its behavior.

Analogy aside, there are three reasons why growth may be the steady state of academic science today:

First, universities are run by professional managers, as well as (sometimes, instead of) professors, thus change is driven not only by educational and scientific goals and faculty interests but also by the goals of professional management. As managers, university administrators benefit from enlargement of their areas of responsibility and are therefore growth oriented. Moreover, organizations benefit from growth to the extent that it enhances their control of essential resources from their environments. Thus there is an internal force toward growth which may be evidenced in incentives for increasing enrollments and research activity. To some degree these forces also underlie the founding of some research centers, which may develop out of administrative initiatives and negotiations with government and industry, not traditional faculty interests.

A second factor is the traditional pyramidal structure of academia, with one professor, a couple of postdocs, and still more grads and undergrads. This structure is demographically feasible only if there is (i) growth, so the ranks below are rapidly growing or (ii) sharp selection criteria, so many are called at each level but successively fewer are chosen. This need to select would reinforce scientists' reliance on putatively meritocratic criteria, such as numbers of publications, citations, and accountability for work proposed.

Third, patterns of the growth of scientific growth, where "way leads on to way," and one stream of research branches and branches again also imply growth in science (see Mulkay et al., 1975 for one such model). But science may not be as dependent on growth as such models imply, for they were devised during a period of rapid growth in science and are colored by the restricted range of examples available at the time as well as by the primacy of explaining the most obvious feature of modern science: its rapid growth. Whatever the reason, we just don't have a good model of science in deceleration or decline. (Kuhn's [1970] idea of overwhelming anomalies leading to paradigm shift is the source of most statements of decline, while others talk simply of the "exhaustion" of interesting problems.

Kuhn's ideas are not helpful here because, in a dialectical sense, matters are "best" in an area when, through the accumulation of anomalies, they seem worst: After all, only then is the stage set for a scientific revolution and the rewards and satisfactions that entails. At bottom, theorists who talk of the exhaustion of problems in an area are saying little more than the playwright's exeunt omnes.)

3. Unintended consequences of policy

Varying the amounts and distributions of inputs through changes in funding policies may have counterintuitive effects. For example, suppose that policymakers wish to free up money to support work in an area of urgency or opportunity (area A) by reducing support for another area (B). On the face of it this is a reasonable plan to reallocate resources. As they do this, they may wish to retain some research activity in B by funding its most capable scientists. (in fact, this reasoning underlies all proposals to enhance the quality of science through increased competition for resources.) Yet precisely because they are the most capable scientists, these individuals may choose to redirect their research away from A to more fruitful areas, leaving behind a pool of scientists who are (on average) less able. Worse, increasing support for area B offers no assurance that only the best scientists will move in. Instead, the quality of research in the area might be diluted by an influx of less able researchers who respond to the opportunity. (See Cohen, 1986 for evidence that increasing support that attracts scientists in the "less prestigious institutions" to work in an area need not increase the area's knowledge base. Hufbauer (1986) and Hackett (1984) offer evidence that major scientific advances may occur prior to major increases of research support.)

The path to a steady state is further complicated when nongovernmental funding agencies become involved. Many welcome the increased participation of industry and the continued support of foundations in the national research effort. But research support from foundations and industry may confound federal efforts to achieve a steady state. Imagine two opposing policies these funding sources might take.

Suppose industry and foundations support work not funded by the federal government. While this is the sort of compensatory behavior we might desire from them, the compensation policy also works at cross purposes with the intent of federal expenditures, acting as a drag on planned change. Ultimately it might create two categories of scientists or universities: those supported by the federal government and those supported by industry and foundations, each with its own research problems, criteria of success, and publication outlets. Whatever else may have occurred, the federal effort to redirect scientists' energies has been blurred by the actions of industry and foundations. (The example is not far-fetched: Blume [1987:13-14] cites NSF data showing how greatly universities may differ in the amount and proportion of research support received from government and industry. Furthermore, Shapley and Roy [1985] argue for a policy to promote a similar bifurcation.)

Now suppose instead that money from these sources accrues to those scientists and universities that also have federal research support (as is now the case). Then industrial support is destabilizing in a feed-forward

fashion: any attempt to meet policy objectives with federal support will be exaggerated by accompanying industry support. This can be distorting and, if we assume that scientists' capacities for doing research are limited (they surely aren't infinitely expandable), it can also be disruptive for it will encourage them to overextend themselves in pursuit of extra-scientific goals (supporting students, augmenting salary, providing charge-out to relieve the strain on university budgets). In effect, successful scientists may become managers of scientific work, counterintuitively driven away from the laboratory bench by their success in research through a perverse variant of the Peter principle.

4. Reactivity

Science is comprised of political actors who may not wish to be steadied. Scientists, centers, research programs within funding agencies, and universities may all be growth oriented. They are proactive in creating conditions for their work, resisting attempts by others to reduce their resources. And scientists, although well-organized into professional societies, have not been especially active in the political arena. But that may change if funding levels are reduced, thus setting in motion powerful political forces. Beyond the undesired, counterintuitive effects that occur almost without deliberate action on the part of scientists and their organizations there may well be a planned, political response to change efforts. ("Pork-barrel" grants to universities, often solicited on their behalf by professional lobbiests, are the most obvious and controversial manifestation of this. Confronted with an uncertain funding environment, some universities resort to political action to secure needed resources.)

The path to a steady state may affect funding agencies' standards for evaluating proposals. As resources are cut, increasing pressure will be placed on accountability (for the specific products listed in a proposal, for example) and on a general product orientation (bean counting mentality about publications). These predictions are supported by my interviews, discussed in part II, and reflect scientists' effort to retain the meritocratic decision making of science in the face of increasingly materialistic forms of accountability. That is, in a bureaucracy it will not do to argue that one scientist's work is "better" than another's in some unspecified way. Rather, the criteria need to be objective, universal, and empirical (thus, scientometric indicators are valued). Yet excellence in science may not be amenable to such forms of accountability. No more so than intelligence has readily yielded to intelligence tests this century. In the extreme this bean-counting mentality may evoke a form of ritualism from scientists, where the "form" of a publication matters more than its content and where risk is anathema.

This emphasis on material products may affect scientists' willingness to take risks, to explore areas that may not be assured of yielding communally-valued results (e.g., Muller, 1980; Orbach, 1986). This is so because if they come up empty-handed their research careers may end because an interruption in their flow of funding may not be recoverable from either a scientific perspective (i.e, remaining at the cutting edge, keeping cell-lines going and grad students at work) or from a career perspective (job security; tenure tied to funding success). But on the other hand scientists also cannot afford to work in very popular areas where they risk

being engaged in priority races because the rewards for second place are vastly smaller than those for being first.

Thus scientists are faced with a dilemma in their efforts to choose problems with appropriate levels of risk and it is very difficult to predict their behavior. Consider this four-fold table that suggests how riskiness of a proposal may be affected by the ease of obtaining research support.

TABLE
Rationales for high and low risk research by ease of funding

	RISKINESS OF PROPOSAL	
MONEY	low	high
tight	be bankable incremental results	low opportunity cost proposal must stand out
easy	keep continuity don't rock the boat	there's plenty: take a flier

The table suggests that when money is tight scientists might prefer low-risk projects because these are "bankable" offering the incremental results that reviewers can evaluate and reward with continued research support. Yet under the same funding condition one can make an argument for taking risks: the opportunity cost is low, since tight money translates into low award rates, there is little to lose by proposing risky work. Moreover, a scientist might reason that it is essential to stand out in the crowd of proposals under such circumstances.

When money is relatively easy to obtain two different rationales come into play, each also supporting high or low risk projects. One might argue that, while money is plentiful, it is best to pursue an integrated research theme; that it is ill-advised to rock the boat (because now the opportunity cost is high: a proposal is likely to be supported if it isn't "offensive" in some sense). Alternatively, the case for writing a risky proposal may be supported by saying that, as there's plenty of money available, one should take a chance for the funding agency has the wherewithal to support innovative work.

The point of this exercise is to show that funding conditions don't necessarily dictate a course of action but that there is room for other unknown contingencies to come into play. Some of these contingencies have to do with the nature of research problems "available" to the scientist, others with the scientist's career and employment circumstances. It is also intended to suggest how difficult it would be, even with a well-developed systems perspective on science, to predict the consequences of attempting to achieve a steady state.

APPENDIX: The Study

Data

This study is based on face-to-face, taped interviews with scientists in their offices. The interview schedule was loosely structured, asking specific questions while allowing time for elaboration and exploration of unique details of a scientist's research interests, employment circumstances, and career. Interviews lasted from one to three hours, averaging about 90 minutes.

Respondents were chosen according to several criteria. One population of potential respondents was identified through lists of grant recipients provided by NIH or retrieved from the Federal Research in Progress data base. These lists were then restricted geographically to the east and west coasts and by "quality" of the department where the scientist worked when the award was made. Quality was determined by the rating of a school's doctoral program in cellular and molecular biology according to the NAS report, "An Assessment of Research Doctorate Programs in the United States: Biological Sciences." I relied on two measures: the scholarly quality of faculty, based on a reputational survey, and the influence of recent publications, determined by counting the publications and evaluating the quality of the journals in which they appeared. Middle-rank schools were within one standard deviation of the mean on these measures; top-rank schools were at least one (and sometimes two) standard deviations above the mean. Geographic restrictions were imposed to keep travel costs and inconvenience reasonable while insuring some diversity. Imposing these quality criteria on the sample allowed the study to focus on active researchers while extending its scope beyond the most prestigious departments.

A second, smaller population of respondents was obtained by asking NSF program managers to send letters requesting an interview to scientists whose proposals were declined funding in one of the three most recent panel meetings.

Respondents also were chosen to provide diversity in academic rank (ranging from full professors through recent tenures to marginal jobs), years of support (ranging from 25 continuous years to first-time investigators), and sex (to avoid underrepresenting women, especially as their positions and problems may differ from men's).

This section is based on interviews with 23 scientists, 2 department chairs, and 4 agency officials. Of the scientists, 14 were in developmental neuroscience, 8 in lymphokines. Five were in low-level, academically marginal positions; 5 were assistant professors, 5 were associate professors with tenure), 7 were professors (although one of these does not have tenure and her career prospects are very similar to the marginals, despite her rank and accomplishments). They are evenly divided between medical schools and academic departments. Their educational backgrounds are generally similar in "quality" of the Ph.D. department--most came from very good schools--but are quite different in baccalaureate origins, ranging from the most prestigious schools to the most obscure.

Their publications and accomplishments varied substantially, as one would expect from a group so diverse in professional age and rank. But even the least accomplished had made noteworthy contributions, judged by the usual measures of publication and citation counts. For example, the first-authored publications of one scientist whose employment circumstances are marginal have received over sixty citations in the 1984 volume of SCI.

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